case of a breakage, it is necessary to wait until the instrument is brought back, which requires generally two or three days. For this purpose a letter attached to the basket containing the instrument tells the finder where to send it, and offers him a reward of 7 francs. It happens sometimes, however, that strong winds carry a line of several kilometers in length, with half a dozen kites and the self-registering instrument away to very great distances. In this connection, it may be interesting to cite two cases in particular where the kites crossed the sea.

In the first instance a west-northwest wind broke the line, and after remaining several days without any tidings of the instrument, the conviction gained that it had fallen into the sea in the direction of Aarhuus, in Jutland, and it was given up for lost. Sometime later a letter was received, which announced that a self-registering instrument had been found to the north of the island of Seeland (or Zealand). At first it was supposed that it was an instrument from a sounding balloon, but when it was received it was found to be one which had broken away with the line about twelve days before. Consequently, the kites, after having fallen into the sea on the east coast of Jutland, had crossed the Cattegat, and gone about 150 kilometers from their starting point.

The second instance is still more curious. After a breakage, in a southwest wind, and several weeks having elapsed without any tidings of the lost line, a dispatch was received from Christiansund in southern Norway, announcing that a kite of a certain number had been found near the coast. On examining the diary of the ascension, it was ascertained that the said kite had been attached to the lost line, but below the self-register; the hope was therefore entertained that the upper kite and the apparatus had been carried still farther away and would still be found, and this also happened. The apparatus having been recovered, the different phases of this journey of more than 200 kilometers, over the plains of Jutland and the Skagerack, could be traced by means of the curves.

A consideration of all the ascensions shows that in these countries it is very easy to attain heights of 1500 to 2000 meters, as the wind is generally sufficiently strong in the lower strata. On the other hand, ascensions to great heights, 3000 to 4000 meters, are relatively rare, notwithstanding all the efforts that have been made to attain them. This would tend to prove that the mean velocity of the wind, which is very great near the ground, approaches more nearly to the ordinary rate in the higher strata. It may also be stated that the difficulties and the cost of materials increase enormously when it is intended to make ascensions in all kinds of weather. In fact, it very often happens that in rather strong winds the tension on the line increases in proportion as the wire is unwound, while the angle of inclination of the wire constantly diminishes. In order to ascend higher we are forced to add another kite, and although care is taken in such cases to provide them with a copper wire "casse" which breaks when the pressure of the wind becomes too great, yet we see the total tension steadily increasing and finally attaining the breaking limit without the kites being detached. In spite of all possible precautions, ruptures are still numerous, and it has happened that more than 14,000 meters of the steel wire, 15 kites, and 3 instruments (which, however, were afterwards recovered) have been lost in less than twenty-four hours. It may, therefore, easily be imagined that under these circumstances the service of continuous ascensions becomes very difficult.

In addition to the kite ascensions, sounding balloons for the exploration of the middle strata of the atmosphere are also sent up. As it has been impossible to obtain hydrogen gas on reasonable terms, the balloons have had to be filled with illuminating gas. This has been found sufficient, as there was no intention of sending them up to great heights. The small area of the country makes it necessary to restrict the duration of the ascensions to twenty minutes or half an hour if one does not wish to run the risk of the balloons falling into the sea, and this is the reason why the balloons attain only relatively low altitudes (5 to 8 kilometers). The gas, manufactured at the gas works of Viborg, is brought in a large reservoir balloon made of cloth and sufficiently heavy to counteract its ascending power. It is brought on a wagon constructed for that purpose, and the gas is passed into balloons made of "papile," sent from the Trappes Observatory and which hold about thirty cubic meters. On an average one balloon is sent up every two days, and in special cases one every day. A clock movement tears them open after a certain interval of time, and a letter promises 14 francs[1] to the person who finds the instrument and returns it. Nearly all the instruments have been recovered, and the curves thus obtained complete the data obtained simultaneously by the kite ascensions. In a great number of cases at Trappes sounding balloons have been sent up the same day, and this gives very interesting points of comparison.

Finally, in cases where there has been no wind for a certain length of time a balloon kite has been used, although very rarely. The illuminating gas does not, however, suffice to make it rise, and it has served especially as a kite having no weight; it has, however, been quite interesting from a technical point of view to confirm the utility of such an apparatus. However, on account of the great difficulties attending the preparation and the maneuvers of this apparatus, we have not been able to study it very thoroughly, and the prevalence of strong winds has rendered its use unnecessary.

The experience gained during nine months of work at this station has led to interesting conclusions from a theoretical as well as from a technical point of view. The tables of the results obtained have been printed promptly in instalments and will be published, it is hoped, a few weeks after the observations have ceased. It has been demonstrated that there would be a great advantage in being able to modify the force of the wind artificially when it is insufficient, but especially when it is too strong. This is the reason why a small maritime expedition in Danish waters is being prepared, which is to conclude this work. The Danish navy has placed a gunboat at the service of the expedition for about three weeks.

On the whole, it may be considered that the work done at the Franco-Scandinavian station is a great step toward the end in view: continuity in observations of this kind. There were some periods of an absolute continuity of seventy-two hours, and several times it was possible to study the different phases of the barometric depressions which passed over that region.

NOTE ON THE RADIATION FORMULAS AND ON THE PRINCIPLES OF THERMOMETRY.

By EDGAR BUCKINGHAM, Bureau of Soils, United States Department of Agriculture.

On page 561 of the MONTHLY WEATHER REVIEW for December, 1902, there are certain statements which, it seems to the writer, should not be allowed to pass without comment. In the second column (lines 23–29) it is stated that "Since J_o is the integral of the area of the curve of energy intensity, it should evidently be greater than J_m under all circumstances, but the fact that by this formula (IV) it becomes less for temperatures above 4119° seems to indicate that there may be something wrong in the deduction of formula III for J_m and II for J_o, from which IV for $\frac{J_m}{J_o}$ was derived."[2] This sentence suggests several remarks.

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[1] About $2.80.

[2] The equations in question are,

$$\text{II. } J_o = \text{const} \times T^4; \quad \text{III. } J_m = \frac{c_1\ 5^5}{c_2\ 10^{5M}}\ T^5; \quad \text{IV. } \frac{J_m}{J_o} = \text{const} \times T.$$

In the first place, the author is basing his work on Wien's formula,

(1) $$J = c_1\, \lambda^{-5}\, e^{-c_2/\lambda\theta}.$$

The theoretical basis of this formula is of no great value, and its originator himself does not pretend that it is.[3] Furthermore, it has been shown very conclusively[4] that even regarded as an empirical interpolation formula, the equation represents the observed values only for a limited range, and fails completely for large values of $(\lambda\theta)$. It would not be surprising, then, to find ourselves led into error by trying to use the formula outside its known limits of application. Of the various equations which have been devised for representing the distribution of energy in the spectrum of a black body, Planck's latest formula,[5]

$$J = c_1\, \lambda^{-5}\, (e^{c_2/\lambda\theta} - 1)^{-1},$$

seems to be the best up to the present time. But let us take the formula (1) as representing correctly the energy J as a function of the wave length λ and the absolute temperature θ. Making this assumption, the other equations in question may be deduced very easily. By taking the logarithms of both sides, differentiating, and equating to zero, we arrive at once at the equation

(2) $$\lambda_m \theta = \frac{c_2}{5}, \qquad \text{(Equation V.)}$$

where λ_m is the wave length for which J is a maximum.

By substituting this value of λ_m in the original equation (1), we get J_m, the value of J corresponding to λ_m,

(3) $$J_m = \left(\frac{5}{e}\right)^5 \cdot \frac{c_1}{c_2^{\,5}} \cdot \theta^5. \qquad \text{(Equation III.)}$$

The equation for Stefan's law may also be obtained from (1), as follows: Since J_o is the total temperature radiation in unit time from unit surface of a black body at the absolute temperature θ, we have

(4) $$J_o = \int_o^\infty J d\lambda = c_1 \int_o^\infty \lambda^{-5}\; e^{-c_2/\lambda\theta}\; d\lambda.$$

Substitute in this equation $x = \lambda^{-1}$, and use the easily obtained integration formula

$$\int x^u\, e^{ax}\, dx = \frac{x^u\, e^{ax}}{a} - \frac{u}{a} \int x^{u-1} e^{ax}\, dx.$$

By carrying this through and taking the value from $x = \infty$ to $x = 0$, the reader will at once find that

(5) $$J_o = \frac{6\, c_1}{c_2^{\,4}} \cdot \theta^4. \qquad \text{(Equation II.)}$$

The author's equations II and III are therefore entirely correct in form. (I have not gone over the arithmetical work, which is of no importance here.) They are mathematical deductions, with no further hypotheses, from Wien's equation (1), and are correct if that is correct, as we are now assuming it to be. From these two equations we at once obtain the equation

(6) $$\frac{J_m}{J_o} = \frac{5^5}{6e^5} \cdot \frac{1}{c_2} \cdot \theta. \qquad \text{(Equation IV.)}$$

Equation IV, then, is correct, in form at least. The difficulty is not in the deductions of the equations, which are well known, but in the author's interpretation of them.

It is perfectly obvious that no matter what the values of the constants in (6) may be, we may make $\frac{J_m}{J_o}$ as large as we please, by making θ large enough. It is equally obvious that J_m and J_o are not of the same dimensions; and confusion has arisen from accidentally overlooking this fact. In more everyday language, and using the common graphical representation, J_m is the maximum ordinate of a curve and J_o is its area. Putting it in this way, any one will admit that there is no necessary relation between the numerical values of these two quantities, or, in other words, that their ratio depends, for a given form of curve, entirely upon the units adopted, and may be made as small or as large as we please by merely changing the unit of length.

To work out the dimensions of all the equations concerned is easy, but requires more space than should be taken here. I will, however, give a table of the dimensional equations in question, which the reader may easily verify at his leisure. For brevity, I use $[W]$ instead of $[m\, l^2 t^{-2}]$ as the dimensions of energy.

$$[c_1] = [W l^2 t^{-1}],$$
$$[c_2] = [l\,\theta],$$
$$[J_m] = [J] = [W l^{-3} t^{-1}],$$
$$[J_o] = [W l^{-2} t^{-1}],$$
$$\left[\frac{J_m}{J_o}\right] = [l^{-1}]$$

A few lines farther on, occur some suggestions as to the propriety of extrapolation, by means of certain formulæ, to temperatures as high as 8000°.

"Temperature" is a much abused word, and the expression "measurement of temperature" is one which is too often used very loosely. There is no such thing as "measuring" temperature in the sense in which the term measuring is commonly used in physics, i. e., there is no such thing as direct comparison of two intervals of temperature which are not coincident at both ends. We have a simple and general criterion for deciding whether the temperatures of two bodies are equal or unequal; and by a further convention we tell, with certainty, what is the sign of the difference in temperature of two bodies. But the mere fact that we speak of the body which cools off as having the *higher* temperature far more often than we speak of it as having the *greater* temperature, betrays the fact that we do not look upon temperature as a quantity measurable in the usual sense.

When we go beyond the bare statement that the temperature of one body is equal to, lower (less) than, or higher (greater) than that of a second body, and assign numerical values to temperature, we are, properly speaking, not measuring temperatures, but *numbering* them. In other words, we adopt some method by which we may assign to each separate temperature a definite number, and there must be a one-to-one correspondence. The method must be unequivocal, in assigning one and only one number to each temperature, and in never giving the same number to two different temperatures. This is the one essential, fundamental principle in constructing a scale of temperature. If we conform to it, we are free in other respects to choose our scale as we please, the choice being, in any case, arbitrary.

One particular scale, Lord Kelvin's, is based upon the two laws of thermodynamics, which are, so far as we now know, general and exact. It is independent of the properties of any particular material substance, and may, therefore, with propriety be called an absolute scale, though other scales might be devised which would be equally deserving of the name. This scale is difficult to use, and for all ordinary practical purposes in physics, it is replaced by another which is certainly not very different from it between the limits of 0° and 100° on the centigrade scale. This is, of course, the international normal scale of the constant volume hydrogen thermometer. There is much talk about temperatures of —200°, etc., "on the absolute scale," but as a matter of fact, we are not *sure* of the value to be assigned to any temperature on Lord Kelvin's scale—the only "absolute" one in use—except within narrow limits, though there we may assign values

[3] W. Wien, Ann. d. Phys., (4), *3*, 530, 1900.

[4] Rubens and Kurlbaum, Ann. d. Phys., (4), *4*, 649, 1901.

[5] Planck, Ann. d. Phys., (4), *4*, 553, 1901. Rubens and Kurlbaum, loc. cit.

with the practical certainty that our assignment is only slightly in error.

It is easily shown by thermodynamics that Lord Kelvin's scale is identical with the scale of that nonexistent body, the ideal gas. This substance is defined by the equations

$$(pv)_\theta = \text{const},$$
$$c_v = \text{const},$$
$$\lambda = 0,$$

where p = pressure, v = volume, θ = temperature, c_v = specific heat at constant volume, and λ is the heat effect during free expansion. The determination of the relation of the international scale, or any other gas scale, to Lord Kelvin's scale depends upon the exact knowledge of the variations of the properties of the gas from those of the ideal gas as referred to above. We have no such knowledge for low temperatures, and therefore we are unable, as yet, to make any positive statement regarding "absolute temperatures" below the centigrade zero.

At the other side of the interval of more or less positive knowledge we are, for a certain distance at least, somewhat better off. We may say that gases appear to approximate more nearly to the ideal state as the temperature rises. Therefore, while waiting for more data, especially on the Joule-Thomson effect at greater ranges of temperature, we may have strong hopes that the international scale, even at high temperatures, is not far divergent from Lord Kelvin's scale, our most secure basis for theoretical work. But the gas scale has an upper limit, imposed by the impossibility of procuring a material for the containing vessel, the bulb of the thermometer. Even by giving up the strict adherence to the international scale and substituting nitrogen for hydrogen, it has only been possible to use the gas thermometer with any approach to accuracy up to about 1450°. Above that temperature it is useless to speak of temperatures by the gas scale, because we can not determine them. It is, of course, probable that in time somewhat more refractory materials will be found, so that the upper limit of the gas scale will be somewhat raised; but to pass over this comparatively small advance that may reasonably be expected, suppose we go at once to temperatures approaching that of the electric arc. Not the slightest prospect is in sight that we shall ever determine such temperatures by the gas scale.

What, then, do we mean when we speak of "extrapolating to temperatures of 7000° or 8000°"? The question whether an extrapolation is or is not "allowable," loses all meaning unless we have some means of defining, physically and not merely mathematically, the quantity of which we are finding the value by extrapolation, and a prospect, however distant, of finding out by *direct experiment* whether the extrapolation is allowable or not. If there is no such prospect, the word "extrapolation" is a misnomer. The extrapolation formula is nothing but a new definition of temperature, so arranged as to coincide with some previous definition throughout the range of that previous definition, but independent and standing on its own merits, outside the limits where the validity of the former definition ceases. Thus, one may with some propriety (though perhaps a rather doubtful one) speak of using the thermo-couple to determine temperatures by "extrapolation," because its range is not so much higher than that of the gas thermometer as to preclude all prospect of the possibility of following the thermo-couple with the gas thermometer; but it is, in the writer's opinion, better, even in this case, to admit frankly that, having decided upon a particular thermo couple, and having adopted a formula which connects its indications, at lower temperatures, with those of the gas thermometer, this couple and its formula define a scale of temperature which is for the present independent, though in no sense absolute.

But at such far higher temperatures as we can produce by the electric arc, all our ordinary scales fail, and there is no prospect of their ever doing any service; we are driven to define temperature in some manner independently of the properties, or even the existence of rigid bodies at those temperatures. The most obvious way of doing this, is to turn to the phenomena of radiation, and then, whatever formula we adopt, that formula, together with the *measurable* phenomena to which it refers, constitute a new and independent scale of temperature which may be made to coincide with some other and more familiar scale at some lower ranges of temperature, but which is, nevertheless, in no proper physical sense an extrapolation.

If such a definition can be based securely, and *with no further assumptions of any sort*, upon the laws of thermodynamics, then it is reducible to, and in a mathematical sense equivalent to, Lord Kelvin's scale. If it is founded upon any other principle or principles as general, as exact, and as independent of the properties of particular substances as those two principles appear to be, then it is a new absolute scale. But unless the scale can be defined in some such manner as warrants our giving it the appellation of "absolute," in the sense in which that term has been used in this note, it remains an arbitrary scale, not reducible to any scale of which we have perfect cognizance at such lower temperatures as are ordinarily within our reach.

Our conclusion is, therefore, that it is not allowable to speak of extrapolation to 8000°. Either the scale is an absolute one, valid and having a definite meaning through all ranges where the laws of physics hold, so that there is no need of extrapolation, or the scale is an arbitrary one. If it is arbitrary, it may, as the scale of the thermo-couple may be made to do, coincide with some other scale within a more limited range, but that does not make it an extrapolation formula. If we are not willing to regard the two scales as independent, we can only consider the one of shorter range as being, in a sense, a special and limited case of the one of wider range.

THE INFLUENCE OF LIGHT AND DARKNESS UPON GROWTH AND DEVELOPMENT.

By DANIEL TREMBLY MACDOUGAL, Ph. D., Director of the Laboratories, New York Botanical Garden.

SUMMARY.

By RAYMOND H. POND, Ph. D., dated Sterling, Ill., May 25, 1903.

In the above-mentioned memoir of over three hundred pages (large 8vo.) Professor MacDougal has recorded the most efficient investigation ever made of the influence of light and darkness upon growth and development. The subjects included are, first, the literature, of which a most thorough study is evident; second, experiments in detail with most admirable and appropriate illustrations, including graphic representation of measurements; third, general considerations, comprising critical discussion of experimental data and their interpretation.

The history of physiological investigation reveals the fact that some attention was given to the influence of light upon growth as early as the seventeenth century, but no epoch-making researches were made until the middle of the last century when Sachs undertook his extensive study of the problem. Since the time of Sachs the literature has rapidly accumulated and the failure of previous investigators to reach the conclusions now demonstrated is to be attributed to insufficient and unreliable data as well as to prevailing ignorance of the phenomena of irritability. During a period of seven years, a large part of which was spent working under especially designed facilities, Dr. MacDougal has perfected the technique and accumulated reliable data. Ninety-seven species of plants representing the various systematic groups and covering a wide diversity of growth conditions have been experimented on.

I.—GENERAL CONSIDERATIONS.

Modes of influence of light upon plants.—As a basis for the